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Hydrology and Earth System Sciences Discussions

HESSD

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Interactive Comment

Interactive comment on "Inferring the flood frequency distribution for an ungauged basin using a spatially distributed rainfall-runoff model" *by* G. Moretti and A. Montanari

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I have a few constructive suggestions to the authors. I know they are genuinely asking relevant questions about a difficult problem, but they do not provide comprehensive answers.

As professionals we all have a responsibility to justify that the flood estimates that we come up are robust and believable. In this paper, for example, the authors did use their professional judgement to dismiss an estimate they obtained by the regionalization approach - they decided it to be too high based on other explanations.

So is it justifiable for them to accept the predictions of their distributed rainfall-runoff

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model? The authors' justification of their new estimates is based on their trust in their model. They test their model predictions using discharge time series from a gauged site, i.e.: (1) the good match they obtain between model predicted discharge time series against observed time series (in the form of regression plots), (2) the thorough sensitivity analyis using GLUE, which seems to raise confidence on the robustness of their predictions.

However, I remain not completely convinced. Even a good Nash-Sutcliffe (they did not even use this) is of course welcome but is no guarantee that the flood peaks will be also right. The model testing should also have been on the event hydrographs for some of the more extreme events - the use of the entire discharge time series would have swamped the key discharge events, and is not a sufficient guide to the model's adequacy.

Secondly, the authors also show good predictions by the model of two flood frequency curves on two internal river gauges. Normally this is a good thing, provided you can extrapolate from these two gauges to an ungauged site. However, since they have already dismissed the regionalization or scaling approach, they are not on firm ground here. What is the guarantee that this match is a good enough indicator of the flood frequency curve at another location? There could be significant differences due to factors that cause attenuation of flood peaks, e.g., catchment area could be big factor: is their model capable of capturing the hydraulic parameters an dprocesses that contribute to this scaling with catchment area? This has not been separately tested, or at least justified.

I know this is a very vexing problem, and I do not want to suggest that the answer is simple or obvious. However, I will have more confidence in their results if they are able to present a few selected flood hydrographs, extracted from their model simulations in the gauged sites, not just Nash-Sutcliffe but actual hydrographs - the more extreme they are the better for me - and convince me that they are predicting the event hydrographs correctly for the correct reasons (Kirchner, 2006).

In terms of statistics, I could additionally suggest that they also extract hydrologically relevant information from a population of extreme event hydrographs predicted by the model - and assemble statistics about runoff coefficients, how the runoff coefficient changes with event magnitude (intensity, duration), the antecedent conditions etc. They could also extract a typical unit hydrograph for these events. With these numbers,they could begin to get a feel for how the so-called annual maximum floods of various return periods are in fact produced: the rainfall excess part, and the routing part, and justify that these are realistic.

I could then suggest other interesting things they could do. For example, with these runoff coefficients, unit hydrographs and antecedent conditions, they could do a number of things. For example, from rainfall analysis, they could estimate a T-year event rainfall (duration, intensity), they could also estimate these from available IDF curves for the region, and then with the use of runoff coefficents and unit hydrographs, they could transform the design rainfall into a design flood of a given T. They could compare the results against the one produced by their model and see how close they get - this is done not in a predictive sense, but in a diagnostic sense.

In other words, what I am suggesting is that: 1) they present results that provide convincing insights into their model performance in predicting flood producing events, not just the entire hydrographs (the latter is no guarantee for the event hydrographs or flood peaks); 2) try to come up with alternative, inuitive and insightful estimates of the T-year floods, e.g., in addition to the method I just described, and the regionalization approach they dismissed, they could also use the classical rational method. 3) once you have a few of these estimates, then it is a question of using hydrological judgement to choose the one you deem is more trustworthy and are able to explain using hydrological judgement. Engineers do this all the time in such situations.

The authors may want to look up the following paper which attempted to do this for a problem involving the estimation of PMF, a much harder problem than the authors are addressing:

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Jothityangkoon, C. and M. Sivapalan (2003). Towards estimation of extreme floods: Examination of the roles of runoff process changes and floodplain flows. Journal of Hydrology, Vol. 281, pp. 206–229.

With the model results they already have, the authors can make the paper more educational for the reader, and make the results more trustworthy and insightful. At the moment it appears like a black box to me.

Finally I have a few detailed comments on the presentation:

1) the abstract is not very informative. It is too diffuse and does not present anything concrete.

2) I had a hard time understanding the geography and location of the catchment. They MUST present a map of the catchment, with the locations of the various gauges clearly marked - the description is hard to follow without a map .

3) The regionalization/scaling relationship they use shows an exponent of -2/3. That seems like a sharp decrease. In Australia this number close to zero, but such a low number is not at all relevant to Italy. What is more relevant however is data from the Appalachian region of the USA - where the exponent is more like -1/3. Can the authors firstly check that the exponent they used is consistent with other similar regions of Italy? Secondly, how would the regionalization work if they used only -1/3. I am tempted to think that in the latter case they will get a more reasonable estimate for the ungaued site, much less than 295 mˆs they are getting now. If so, then it cannot be dismissed any more. It is worth checking. Please see the following paper for the Appalachian results:

Robinson, J. S. and M. Sivapalan (1997). An investigation into the physical causes of scaling and heterogeneity of regional flood frequency. Water Resources Research, Vol. 33, No. 5, pp. 1045-1059.

In conclusion, these comments are not presented as criticism, but as ideas for further analysis. I strongly believe there is considerable room for the use of intuition, built on understanding and experience, rather than totally relying on sophisticated (but complex) models and sophisticated evaluation tools such as GLUE. The annual maximum flood peak is one number out of 8760 hourly data points in a year - it is like "searching for a needle in the haystack". Therefore, the analyses and evaluation should be more targeted towards those processes that actually cause (extreme) floods.

The suggestions I make do not necessarily mean lots of new work - they mostly involve presenting more targeted results and carrying out more targeted analyses on the simulations they have already done.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 1, 2008.